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INTRODUCTION

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THE Conference on Business Concentration and Price Policy was held at Princeton University, June 17-19, 1952. The conference was organized, under the sponsorship of the Universities–National Bureau Committee for Economic Research, by a steering committee consisting of Corwin Edwards, Carl Kaysen, Edward S. Mason, George J. Stigler, chairman, and Clair Wilcox; and Gideon Rosenbluth was the (I must gratefully say, highly efficient) secretary. The papers and discussions in this volume have been revised since they were delivered.

One task that economists have long taken seriously is that of explaining what determines the behavior of an industry. Under what conditions do prices fall (or rise) with expansion of output? How does the industry change its methods of production in response to changes in prices of inputs? When will customers be classified and each class asked to pay a different price? Will relatively large profits lead to an increase in the number of firms and, if so, how rapidly? Such questions—to which many economists would like to add less-studied questions such as: How does industry structure affect the rate of technological advance?—are at the center of modern economic analysis.

When one turns to the empirical investigation of such questions, he must at the outset determine what industries are. (Most empirical studies have dealt almost exclusively with manufacturing industries, and we shall follow this regrettable tradition here.) Almost invariably the empirical workers have accepted, perhaps with minor modifications such as Rosenbluth applies to the Canadian data, the practices of the census officials who compile the data. They cannot be blamed: the task of appraising the relevance of census classifications to questions of industrial organization is so vast that it would swallow up any more specific investigation the economist had in mind.

Moreover, it is not obvious that the census classification is inappropriate to our interests, which here center in questions of competition and monopoly. Conklin and Goldstein summarize the principles of the present classification. These principles rest fundamentally upon similarity of products or production processes of establishments (plants) which are to be combined into one industry. Particular

attention is paid to supply conditions, and "the industry generally represents a group of close competitors, producing close substitute commodities." Homogeneity is sought: most of the products attributed to the industry must be accounted for by the establishments in the industry, and most of the products of the establishments in the industry must fall into the industry defined by its major products. The fact that in the application of such rules to specific industries, census bureaus are influenced by opinions of businessmen, demands of other government agencies, etc., insures that the classifications are not insulated from business experience and public policy.

Yet the rules are intrinsically ambiguous. Economists have written acres on the problem of defining the closeness of substitute products, and no doubt problems of equal complexity are encountered in estimating the similarity of production processes. The influence of business attitudes and public policy is also not an unmixed blessing, for the classifications that are germane to taxation, labor problems, tariffs, and the like are not necessarily suitable to the analysis of problems of competition and monopoly.

Price theory has certain direct implications for this problem of defining industries that have not received adequate recognition in official practice, and they deserve at least brief comment here. An industry should embrace the maximum geographical area and the maximum variety of productive activities in which there is strong long-run substitution. If buyers can shift on a large scale from product or area B to A, then the two should be combined. If producers can shift on a large scale from B to A, again they should be combined.

Economists usually state this in an alternative form: All products or enterprises with large long-run cross-elasticities of either supply or demand should be combined into a single industry. In this form it is perhaps forbidding to the statistical worker, for generally cross-elasticities are calculated from empirical equations relating quantity supplied or demanded of one commodity to a host of prices, and we do not have—and cannot in the reasonable future expect to get—enough of these equations to base a general census upon them. But much more feasible methods of detecting substitution exist. If establishments making wooden office furniture in one year shift in considerable numbers within a year or two to making metal office furniture, this is conclusive evidence of high supply substitution—and can be measured with information now

collected but not published. If numerous buyers of cardboard shipping containers in one year are found to be buyers of wooden or burlap containers soon thereafter, this is conclusive evidence of high demand substitution—and can be measured with the type of information now collected.

One important application of the rule of high substitution is to international trade. If a commodity is on either an export or an import basis, its concentration should usually be measured for a market larger than the domestic area. If the commodity is on an export basis, foreign buyers have alternative sources of supply, which must be included in the "industry"; if it is on an import basis, domestic buyers have alternative domestic supply sources, which again should be combined with the foreign supply. In either case it is necessary to take account of the industry structure abroad, but this extension of the area of work of the Bureau of the Census may not be objectionable to its staff.[1]

Once supplied with the frequency distribution of firms by size within properly defined industries, how shall we measure concentration? The large-scale statistical studies have so far employed measures that are directly formed by the disclosure rules of censuses. Despite Rosenbluth's welcome assurance that it does not seem to matter whether we take the proportion of the industry's output coming from the largest three, four, or other small number of firms or the number of firms required to account for, say, three-quarters of the industry's output, we can be certain of one defect in the calculation of these measures. It lies in the time period.

One of Marshall's greatest contributions to economics was to show that calendar time units are seldom a proper basis for measuring economic forces and to elaborate a schema of short- and long-run periods which were defined in terms of the forces which dominate them. The classification was especially relevant to competitive industries because long-run forces can usually be assumed to be negligible in the short run. Under monopoly, however, long-run forces may be decisive even in the short run because the monopolist reaps a large share or all of the future effects of his current policies. A good concentration measure (like a good industry concept) should relate to the long run. This has always been recognized

[1] The problem of the market area is greatly complicated if tariffs or quotas permit price discrimination between domestic and foreign markets, except in the extreme case when the two markets are completely independent and must be treated separately.

implicity; no one has ever said that concentration rises in late afternoon as eastern factories close down.

The long run is defined in terms of the period necessary for specified changes to take place, and two such changes or forces seem specially significant in studies of monopoly. One is on the demand side: How long a time is required for buyers to move along their long-run demand curves? If buyers can make fairly complete adjustments to prices only by such time-consuming procedures as moving their plants or radically modifying them, this period may be of several years' duration. The second force is on the supply side: How long a time is required for outsiders to detect large profits and to enter the industry? Normally I would expect this period to be at least as short as the demand period (although this conjecture has hardly any empirical basis) if no conventional barriers to entry, such as patents and raw material control, exist. Clearly calendar length of the long run may vary widely among industries.

Since the long-run forces may require a fairly long period of calendar time to work themselves out, one might infer that concentration measures should be calculated for periods of, say, five years. But against this must be put the consideration that the long run does not always completely dominate the short run: it may be sensible, for example, to behave monopolistically for a few years and then to lose one's monopolistic position. It is one of the tasks of empirical research to determine the relative roles of these arguments,[2] and I should think that we ought to have concentration measures calculated for three, five, and possibly even ten years, as well as for the inevitable one year.

The other question remains: Which parameters of the frequency distribution of firm sizes are relevant to the behavior of the industry? The relative output of a small number of firms, which is now used, is surely one relevant parameter, although most of the proof for this assertion still lies in the formal theory of oligopoly and not in empirical studies. Until we get the empirical studies, we are not likely to progress far in the refinement of concentration measures. It is easy enough to introduce additional parameters whose relevance to the behavior of the industry can be plausibly argued: for example, I would expect to find also that the absolute number of firms and the degree of instability in the shares of the

[2] From the viewpoint of social policy, short-run monopolies are of course much less important, and—with present durations of antitrust court actions—almost beyond control.

largest firms from year to year were important in influencing the industry's behavior. But plausibility is at least as much an effect of skillful argument as an evidence of probability of truth in an area as complicated and unexplored as oligopoly, and there is little point in multiplying such parameters at the present time.

Scitovsky properly emphasizes the fact that our interest in concentration is not restricted to its effects on the allocation of resources among firms—the traditional focus of the theory of monopoly. We are also interested in the effects of concentration on the distribution of income, the distribution of political power, the efficiency with which resources are used within the firm, etc. Wholly different measures of concentration may be called for in the empirical study of these other facets of the monopoly problem. The relationship of monopoly to the distribution of income, for example, surely involves the absolute sizes of firms and the distribution of ownership of monopolies. Important as these other problems are, however, one may still argue that the traditional focus on the power of the firm in the market is basic, for if this power is absent all the other problems vanish (as monopoly problems).

Miller examines an important deficiency in the traditional theory of monopoly, from which the concentration studies stem. The neoclassical theory of competition and monopoly was developed with two paramount objectives: to provide a clear and consistent theory of economic behavior, and to be analytically manageable. Both of these objectives were met fairly well by defining competition in terms of a stationary economy—one in which consumers' tastes, production techniques, and productive resources were stable through time. The resulting theory is immensely useful in a wide range of economic problems; but it is not directly applicable to problems raised by economic development—the rapid growth, and frequently unpredictable changes, in consumer demands and productive techniques and resources.

Schumpeter has sketched with great brilliance the possible paradoxes in applying the theory of stationary economies to historical developments.[3] The argument is weighty and clearly poses the problem of constructing a definition of competition which is suited to firms in a changing economy. Schumpeter's "solution," which was to label as monopoly all departures from perfect competition in a stationary society, is not useful. Real progress in this area seems also

[3] Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (Harper, 1942), Chaps. VI-IX.

to demand that incisive empirical work for which economists so monotonously beg. But I suspect that when our knowledge of economic growth has increased, we shall not be called upon to reverse all the conclusions reached by stationary analysis.

Let us put aside these and other possible complications and return to the concentration of industry. Industries vary greatly in their concentration, and we naturally seek to learn why. Three papers in this volume deal with important forces affecting concentration: the economies of scale, mergers, and taxation.

Since Cournot's time it has been recognized that at most only a few firms can usually survive in an industry in which the average cost of production of a firm declines as its size increases, and even today this is the popular explanation for concentration. Smith has summarized recent work on this subject, including the interesting efforts to estimate the costs of firms of different sizes from technological relationships between inputs and outputs. This latter type of approach has many attractions, especially in studies of the social (in contrast to private) economies of scale. But it also has two major defects. One defect is generally recognized: the method cannot be extended to selling, recruiting labor, financing, etc. Another defect is not always recognized: the economist is "solving" the problem of measuring economies of scale by turning it over to someone else, and yet it is fundamentally an economic problem.

Smith shows how difficult the problems of measurement are, even though he does not emphasize the point (the valuation of inputs) that I find most troublesome. Difficulty is not an adequate reason for abandoning a problem, but I think there are some positive reasons for determining economies of scale from changes in concentration over time rather than using economies to explain concentration. That is, those firm-sizes whose outputs are growing relative to the industry may be interpreted as having the lowest (private) costs. All comprehensive definitions of economies of scale ultimately imply that firms with the lowest costs prosper relative to other sizes of firms, and it is desirable to recognize this explicitly by defining the most efficient size as that which grows relative to other sizes. This interpretation does not inhibit research on the factors that lead to concentration, for one may still investigate the influence of plant size, of advertising, of the nature of the product, etc. In fact, I would consider it a merit of the reformulation that it divides a vast and complex problem into a series of more specific and manageable problems.

The merger of firms within an industry (putting aside "vertical" mergers) has been a major force in changing the firm structure of many industries. Mergers provide an interesting problem in measurement because of the peculiar basis for recording them: mergers leading to large firms, and acquisitions of all sizes of firms by firms that are already large, constitute the elements of newsworthiness which leads the financial press—and thus the historians of mergers —to record them. Absolute size, moreover, is commonly the test of "large" firms, although size relative to the industry is more relevant to questions of industrial structure. Because large firms are necessarily relatively few in number, perhaps on the order of one merger in a hundred is recorded, and the proportion no doubt varies through time and among industries. The available historical series on mergers, given by Markham, are thus sketchy to an extreme: long periods (before 1887, 1904-1918) have not been studied; size of firms has not been studied, or always recorded, for the variable points of truncation in reporting; horizontal and other mergers have not been distinguished; the industrial composition, when reported at all, is very crude; etc.

Much of the uncertainty over the causes and effects of mergers is attributable to this lack of information. For example, some economists believe that the improvements in transportation in the decades after the Civil War were an important factor in bringing mergers—chiefly because the expansion of market areas increased competition. Simple tests of the hypothesis are easy to devise: mergers should have come earlier in commodities whose production was geographically localized and in products (like ships and jewelry) for which transportation costs were relatively unimportant. We cannot, with our present empirical material, apply such tests.[4] A systematic recompilation of the historical record would be a vast task, but it would be labor well spent.[5]

Fifty years ago only one class of taxes was ever mentioned in studies of industrial organization: tariffs, "the mother of the trusts." Now taxes insist upon intruding into every branch of economic

[4] One basis for my belief that this particular explanation of mergers will not be found useful is that England had a wave of mergers at approximately the same time, although its regions became close-knit considerably before ours.

[5] When it is undertaken, particular attention should be paid to the timing of the several steps in mergers. The period between the initiation of negotiations and the formal merger or acquisition may be variable but substantial—indeed this is one reason, I believe, why the time series show considerable erratic short-run fluctuation.

analysis, and in a variety of forms and with a labyrinth of technical details sufficient to discourage casual generalization. Lintner and Butters' survey of the effects of income and estate taxes reveals the possibility that many significant influences on the industrial structure now flow from these taxes, and the same can be said of certain excises, payroll taxes, etc. Students of public finance may not welcome this new facet of their work: it is hard enough to devise a tax that will raise substantial revenue, be allocated equitably, counteract cyclical fluctuations, encourage efficiency and innovation, and keep the party in power, without adding a due regard for the preservation of competition.

One feature of the taxes Lintner and Butters discuss is that their effects appear to be related usually to the absolute rather than to the relative size of firm. The effects of these taxes upon concentration would therefore appear to be more powerful in industries in which the relatively large firms are large in absolute size, for then substantial absolute growth is required of new rivals before they can offer important competition (and it is absolute growth that taxes may retard). With this in mind it is interesting to compare changes between 1935 and 1947 in concentration in manufacturing industries classified by the absolute sizes of the largest firms. It would be preferable to measure size by assets in this connection, but value added is the most relevant measure available. The results of this tabulation are given in the accompanying table. The decline in con-

TABLE 1

Concentration Ratios in Manufacturing Industries, 1935 and 1947, Classified by Average Value Added in the Four Largest Firms, 1935

Value Added per Firm in Four Largest Firms, 1935	*Number of Industries*	*Per Cent of Value of Product Produced by Four Largest Firms (concentration ratio)*	
		1935	*1947*
Under $250,000	7	36.5	29.5
$250,000- 500,000	13	42.0	39.8
500,000- 750,000	14	41.7	37.7
750,000- 1,000,000	10	36.4	33.8
1,000,000- 2,500,000	37	43.4	42.3
2,500,000- 5,000,000	23	43.1	41.1
5,000,000-10,000,000	16	52.6	51.0
10,000,000-25,000,000	7	63.8	66.3
25,000,000 and over	4	70.7	70.7
Total	131		

Source: *The Structure of the American Economy*, National Resources Committee, 1939; *Concentration of Industry Report*, Dept. of Commerce, 1949.

centration was substantial in industries where the largest firms are absolutely small, but no decline occurred at the opposite end of the scale. Taxation may have been one of the significant influences in this pattern of change.

In the essays of Adelman and Edwards we leave the subject of horizontal concentration and explore other aspects of industrial structure. Adelman estimates by several procedures the quantitative extent of vertical integration in different firms and industries. We customarily view vertical integration as a technological problem: if a firm or plant produces an input it previously purchased, we say it has become more fully integrated. One difficulty with this approach is that it is applicable only if technology is stable. The National Bureau of Economic Research recently turned over publication of its books to the Princeton University Press, so we may say that it is now less vertically integrated. But if its researches should be televised—I have been informed of no such plan—no degree of program preparation corresponds to publication, so the change in vertical integration is indeterminable.

A quantitative measure of vertical integration comparable among industries must be monetary in nature, and the most common such measure is the ratio of value added (roughly, receipts minus purchases of materials) to value of product. This measure pertains only to intra-establishment integration when it is calculated from census data, and even then is subject to two serious ambiguities. The first is that when a plant produces a variety of products, with different ratios of value added to value, the extent of vertical integration varies with the composition of output, even if production processes do not change. The second ambiguity is that census industries frequently contain plants engaged in successive operations—motor vehicles is an extreme example—so the value of product contains much duplication.

Rather than enter into the problem of dealing with value-added data, we shall be content to notice that occasionally the Census of Manufactures reports information that is directly relevant to the extent of vertical integration. In each census, for example, there are reported the quantities of some commodities made and consumed in the same establishment and the quantities made for sale. Sample figures may be reproduced:

Year	*Production of Sulfuric Acid, 50° Baumé, tons*	*Per Cent Made and Consumed in Same Establishment*
1909	2,764,455	46.5
1919	5,552,581	40.0
1929	8,491,114	31.5
1939	7,711,487	33.2

It might be possible to make a general analysis of vertical integration by recasting census data in this form.

One may argue—as I would—that the fundamental basis of power of the conglomerate firm that Edwards describes is monopoly in the conventional sense. Some of the phenomena he describes are illustrations of well-known theoretical propositions, such as that individual monopolists of goods complementary in demand will make smaller aggregate profits if they act independently than will a single monopolist who takes account of the interrelationships in demand. Again, many aspects of the large, diversified firm's activities seem explicable if there are substantial difficulties in accumulating large amounts of equity capital; the capital market for small firms is possibly a strategic factor in a vast array of industrial practices.

But whatever its basis, the conglomerate firm poses new problems also. When large firms are cooperating in relatively concentrated industries, will they not also tend to cooperate in other industries where concentration is so low that normally competitive behavior would have been expected? Conversely, if the firms that are large in one industry are medium-sized or small in other industries, may not their differences in activity raise substantial difficulties in arriving at agreements in any one of the industries? We need to know which of the infinitely many possible constellations of related concentration in several industries are of empirical significance and to analyze their workings in detail.

Price behavior is so important an aspect of industrial behavior that apology is required, not that the conference had several essays in this area, but that there could not have been more. Still, the full-cost principle, price rigidity, and price discrimination are all sufficiently important, so the conference planning committee cannot be accused of gross neglect. I shall restrict my remaining comments to a problem on which all of these papers touch: How can one measure the relationship of price behavior to the structure of an industry and, in particular, to its concentration?

The output of a competitive industry is such that price equals

long-run marginal cost. It is therefore natural to measure, or possibly even to define, the departure of an industry's price from the competitive level by the ratio of price to long-run marginal cost. Useful as this ratio is in the analysis of individual industries, there is unfortunately no known method of making tolerably comparable estimates for many industries so that one might correlate this measure with concentration data. For such broad surveys one is forced to employ a substitute measure.

The temporal rigidity of prices, with respect to either frequency or amplitude, has been the most popular substitute measure, and the vast literature on its use is discussed by Ruggles. For a time, price rigidity was hopefully taken as good evidence of noncompetitive behavior, but both economic theory and statistical studies have greatly weakened the confidence in this evidence. Monopolies may charge flexible prices in their own interests, and competitive industries may have periods of stable supply and demand conditions. The statistical work has shown little correlation between rigidity and concentration, but this work has been plagued by problems of data. Price data and production data are usually collected by different agencies, so that one must use (quoted) prices that are uncertain samples of the industry's real price structure or values of output which are influenced by changes in the composition of output.[6] Extreme price rigidity is inconsistent with competition, but beyond this the association is at best weak.

Since persistent and systematic price discrimination is also invariably associated with noncompetitive behavior of an industry, one might look to this area for measures. Yet price discrimination takes on a considerable variety of forms—geographical, product class, customer class, etc.—as the reader of Machlup's essay will be persuaded. (Machlup is apparently more optimistic than I on the possibility of also disentangling the motives which lead to price discrimination.) Some of the forms of price discrimination deserve study as possible bases for measures of noncompetitive behavior; in particular, the comparison of domestic and export prices might be feasible for a considerable number of industries.

The full-cost principle, in the only one of its numerous versions that I shall consider, states that prices are set equal to average variable cost plus a stable markup per unit of output. If this hy-

[6] Census value-of-product figures are possibly seriously biased because two components of output—interplant transfers and inventory additions—are often valued at cost.

pothesis is correct, we have a new measure of noncompetitive behavior because no competitive industry can adhere to such a formula in the face of large fluctuations in output. But if this theory is correct, then the conventional theory of imperfect competition, which makes demand a factor in price determination, is incorrect, and this dispute must first be settled. The settlement—presumably by a recourse to empirical tests—is difficult for two reasons. One is that the full-cost theory has many versions in addition to that stated above, as Heflebower shows, and its sponsors also differ considerably among themselves on the factors governing the markup on variable costs. The other difficulty is that both the conventional theory and the full-cost theories generally agree on the most easily tested predictions: the general correspondence between movements of price and costs, the similar movements of profits and output, etc. They differ qualitatively only in that the conventional theory affirms, and the full-cost theories deny, that short-run changes in demand will affect selling price even though prices of inputs do not change.[7] It is to be hoped that proponents of the full-cost theories will soon test its ability to predict price movements as compared with the conventional theory.

These various measures of price behavior do not of course constitute a complete listing.[8] The available measures, however, are usually either difficult to quantify or ambiguous in interpretation, and much work remains to be done here. Progress in the measurement of price behavior as an index of performance will in turn contribute greatly to improvements in the measurement and interpretation of concentration, for our interest in concentration is centered in its effects on the behavior of industries.

[7] Certain less prominent differences also exist. For example, on the conventional theory a monopolist is less concerned over the effect of current price on future demand if the commodity is perishable, whereas the full-cost theories make no such distinction.

[8] In addition to the popular measure, the ratio of price to average cost (or profitability), interesting experiments have been made with differences among firms in the level, and time and direction of change, of prices.